HETEROLOGICALITY

By Leon Bowden

IN a recent number of ANALYSIS[1] Prof. Ryle very thoroughly examines Grelling's paradox and concludes that it and others are due to treating referring expressions as fillings of their own namely-riders. Although I find his explanation convincing I feel that, in view of the erroneous doctrine which has arisen especially from this paradox, and because of its intrinsic interest, it is not redundant to make his main point in another way. Consequently, the following is best read as an appendix to his paper.

The removal of the air of paradox depends upon showing that certain statements of similar grammatical form are, despite the similarity of their wording, vastly different in their logic. That the paradox has arisen is itself evidence that we are somewhat unfamiliar with the logic of this neologistic and rather artificial concept, heterologicality. It is therefore desirable first to consider a familiar concept which has a similar logic. Such a concept is primeness (as used in mathematics). By first examining a fallacious deduction in which the fallacy will be obvious because the concept is familiar we shall attempt to make obvious the similar fallacy relating to the less familiar concept. Thus as a preliminary to contrasting the logic of such statements as " ' un-hyphenated ' is polysyllabic " and " ' un-hyphenated ' is heterological " we shall consider the contrast in logical force of such statements as " three is odd " and " three is prime ".

Suppose that a foreigner, but slightly acquainted with English, is told that the statement " five is not divisible (exactly) by two " follows from the statement " five is odd ", and that the statement " seven is not divisible by two " follows from the statement " seven is odd ". Upon being told " three is odd "he might well conclude that three is not divisible by two ; and if told " four is not odd " he might perhaps conclude that four is divisible by two. Further, if told that the statement " five is only divisible by five " follows from the statement " five is prime ",[2]

[2] The usual mathematical practice in number theory, which is followed here, is to restrict the prime number field to 2, 3, 4, 5, . . . , omitting 1 from the divisors (but not from the quotients). This enables a prime number to be defined as " a number which is divisible only by itself ", otherwise, since every whole number is divisible by one, it would be necessary to add the tiresome qualification " and by unity ".

upon being told " Three is prime " he would probably conclude, (if he does not know what numbers are denoted by ' three ' and ' five '), that three is only divisible by five. Of course there is a fallacy here. How would our foreigner attempt to justify his arguments ?

Supposedly he would argue as follows. Five, seven, and three have in common the property of being odd, whatever this property may be. Since neither five nor seven is divisible by two by virtue of this common property, three likewise, in virtue of this common property is not divisible by two. Similarly, since five and three are both prime they have in common the property of being prime, no matter what this property may be; and as five is divisible only by five by virtue of this common property, three likewise has the property of being divisible only by five.

Now let us consider the fallacy. In virtue of the definition of odd (number), to say " five is odd " amounts to saying that five has the property of not being divisible by two, and to say " seven is odd " amounts to saying that seven has the property of not being divisible by two, and similarly for three and four. In short, to say " three is odd " is to say the same thing about three (or to ascribe the same property to it) as to say " four is odd " is to say about four. But to say " five is prime "amounts, as we all know, to saying that five is divisible only by five (and not by three), whereas to say " three is prime " amounts to saying that three is divisible only by three (and not by five). Thus to say " five is prime " does not amount to saying the same thing about five as to say " three is prime " amounts to saying about three.

Because three and five are both prime, it is tempting, and, but for our familiarity with the concept, would be misleading, to say that they have a common property. Three is prime partly because it is divisible by three, while five is prime partly because it is not. Three is prime partly because it is not divisible by five, while five is prime partly because it is. Thus we are forced to say that three and five have in common the property of being prime partly because each has a property which the other lacks.

Let us call such properties of whole numbers as being divisible (or not divisible) by two, by three, by four, . . . , ordinary number-properties. Then to say that two numbers have, in common, the property of being prime, amounts (partly) to saying that there are ordinary number-properties which they do not have in common (and partly to saying that there are ordinary number-properties which they do have in

common, *e.g.* neither three nor five is divisible by two, or four, or six, or seven, or eight, or nine, . . .). To avoid prolixity of statement we shall for the moment ignore this latter group of ordinary number-properties mentioned in the preceding bracket. This will not invalidate the central point, but on the contrary focus full attention upon it. Granted this proviso, we may say that three and five have a common property only in the sense that each is divisible by itself. But the property of being divisible by itself is not an ordinary number-property. Run through the list of ordinary number-properties indicated above, and we come to the property of being divisible (or not divisible) by one million and six, and later, by seven billion and eleven, but we never, even in principle, come to the property of being divisible by itself. There is no such number.

This point may be made in another way. Because from the statement " five is odd " the statement " five is not divisible by two " follows, and because from the statement " seven is odd " the statement " seven is not divisible by two " follows, our foreigner supposes that from the statement " three is odd " the statement " three is not divisible by two " follows. That is, he tacitly assumes that from any statement of the pattern " . . . is odd " there follows a statement of the pattern " . . . is not divisible by two " (where both gaps are filled by the same numeral). Here his assumption is correct by virtue of the definition of odd (number). Likewise by virtue of the definition of prime (number), from a statement of the pattern " . . . is prime " we can proceed to a statement of the pattern " . . . is only divisible by itself " (where both gaps are filled by the same numeral), e.g. from " five is prime " follows " five is only divisible by itself ", and from " three is prime " follows " three is only divisible by itself ". Here, in the latter context " itself " refers to three, but in the former it refers to five. Our foreigner tacitly supposes that to say " five is only divisible by itself " is to say the same thing about five as to say " three is only divisible by itself " is to say about three. To do this is, as Prof. Ryle puts it, to treat a referring expression as a filling for its own namely-rider.

The fascination (or exasperation) of the heterological argument lies in its air of paradox. The predicate end of the sentence " ' heterological ' is heterological " looks like a philological epithet of ' heterological ' doesn't it ? To quote Prof. Ryle " It's a predicate of a quoted expression, isn't it ? ". But we have just seen that to suppose that such sentences as " three is odd " and " three is prime " have the same logical grammar (as their

'grammatical' grammar would suggest), is to be led into mistaking a referring expression for a filling for its own namely-rider. To say " There are an infinite number of twin even numbers " (such as the pairs two and four, or ten and twelve, or eighteen and twenty, . . .) sounds grammatically very much like saying " There are an infinite number of twin prime numbers " (such as the pairs three and five, or eleven and thirteen, or seventeen and nineteen, or twenty-nine and thirty-one, . . .), but these statements are nevertheless of vastly different logical complexity. Although the truth of the former statement is supremely obvious, the more than two thousand years since Euclid proved that there are an infinite number of primes has not sufficed to prove or disprove the latter.

We can now re-write the above, substituting statements such as " ' un-hyphenated ' is polysyllabic " for those such as " three is odd ", and statements such as " ' un-hyphenated ' is heterological " for " three is prime ", and suppose our foreigner to make similar inferences, whereupon similar criticisms apply.

Especially we recall what was said about the property of being prime. ' Un-hyphenated ' is not un-hyphenated, so that ' un-hyphenated ' is heterological. ' Hyphenated ' is not hyphenated, so that ' hyphenated ' is heterological. Thus ' un-hyphenated ' and ' hyphenated ' both have the property of being heterological partly because the one has and the other lacks an ordinary philological property, namely that of being un-hyphenated. If we are to say that being prime is an extra-ordinary number-property, then with equal justification we are to say that being heterological is an extra-ordinary philological property.

An epithet is heterological if it lacks the property for which it stands. Given that the statement " ' un-hyphenated ' is a word containing a hyphen " follows from " ' un-hyphenated ' is heterological ", to infer " ' monosyllabic ' is a word containing a hyphen " from the statement " ' monosyllabic ' is heterological " is to assume, tacitly, (analogously to the " . . . is only divisible by itself " example) that " . . . lacks the property for which it stands " mentions an ordinary philological property, or alternatively put, that this referring expression is a filling for its own namely-rider.

The point may be generalised. Any argument involving extra-ordinary properties, numerical or philological, pre-supposes that there are ordinary properties, numerical or philo-logical, to which reference is made. That is, a statement of the pattern " . . . is prime " or " . . . is heterological " such as

"two is prime" or "'monosyllabic' is heterological" is exponible by a statement or conjunction of statements such as "two is divisible by two" and "two is not divisible by three" and "two is not divisible by four", etc., or "'monosyllabic' is not a word of only one syllable" where the sentence fragment, such as "... is divisible by two" or "... is not a word of only one syllable" is the definiens or the denial of the definiens of a PRESUPPOSED property epithet, such as "... is even" or "... is monosyllabic". Let us call the definiens of a presupposed property epithet a primitive epithet. Then, symbolically, if 'a' and 'b' are of the same logical type e.g. numerals or philological epithets and 'ψ' is primitive epithet, then an ordinary epithet 'ϕ' is characterised by the fact that, if

$$\phi(a) \equiv \psi(a) \qquad \ldots \quad (1)$$

is true, then
$$\phi(b) \equiv \psi(b) \qquad \ldots \quad (2)$$

is true also, whereas if 'ϕ' is an extra-ordinary epithet it is characterised by the fact that if (1) holds (2) need not hold. Discovery of an instance (2) which does not hold indicates that 'ϕ' is an extra-ordinary property epithet. Clearly 'prime' and 'heterological' are extra-ordinary property epithets. Since an epithet cannot be both an ordinary and an extra-ordinary epithet, let us call them A-epithets and $\bar{\text{A}}$-epithets (to be read 'not-A') respectively.

To conclude, in view of the foregoing, the paradox is stated with logical appropriateness as follows :

Suppose 'heterological' is heterological, then 'heterological' does not have the property (A-property) for which it stands. But, the property (A-property) for which it stands is the property ($\bar{\text{A}}$-property) of being heterological, that is, 'heterological' does not have the property of being heterological. However, if 'heterological' is not heterological it has the property (A-property) for which it stands. But, the property (A-property) for which it stands is the property ($\bar{\text{A}}$-property) of being heterological, that is, 'heterological' is heterological.

To obtain the paradox we have to use, twice, the flat contradiction that the A-property for which 'heterological' stands is an $\bar{\text{A}}$-property. Russell's contradiction is amenable to similar treatment.

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ON CAREFUL REASONING IN ORDINARY LANGUAGE

By Warren C. Haggstrom

1. LET us consider the class of predicates P in ordinary language such that P is vague. In other words let us consider the class of predicates P in ordinary language such that for many things t[1] we don't know whether t is a member of the class characterized by P or not. It is well known that it is difficult to reason carefully while using predicates such as P. The question arises : what can be done to lessen this difficulty ?

One suggestion has been to assign to P a more precise meaning or to specify a determinate class of things which is said to be the class which is characterized by P. Usually this class is too large to be specified by enumeration of its elements. Usually it is specified by equating P with another predicate (or combination of predicates) Q such that Q determines quite exactly a class of things and that class of things can plausibly be said to be determined by P.[2] Let us call the proposal to equate P with Q an analytic definition in which P is the definiendum and Q the definiens.[3]

2. One aid to reasoning which is supplementary to the above suggestion is as follows.

For each P there is usually more than one definiens, each of which determines a different class. Generally, each Q will have one of three relations with P :

(1) Q definitely implies P.[4]

(2) Q definitely is implied by P.

(3) Q neither definitely implies P nor is definitely implied by P.

There is no Q such that both Q definitely implies and is definitely implied by P, since by hypothesis Q is more precise than P.

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[1] In this usage, ' t ', of course, may represent linguistic as well as non-linguistic things.

[2] That is, it would be plausible to those people using P that P determines the class of things determined by Q.

[3] This meaning of ' analytic definition ' is approximately that which is described in Ambrose and Lazerowitz *Fundamentals of Symbolic Logic*. pp. 37–38 ; 40–41.

[4] By ' Q definitely implies P ' I mean approximately that for any user of the ordinary language in question, any member of the class characterized by Q is definitely a member of the class characterized by P. This concept of " definite " implication neither is nor is intended to be particularly precise.

If a definiens definitely implies P we will call it a sufficient definiens for P.

If a definiens definitely is implied by P we will call it a necessary definiens for P.

If a definiens neither definitely implies nor is definitely implied by P we will call it an associative definiens for P.

Thus each definiens for P must be either a sufficient or a necessary or an associative definiens for P.

Suppose we ask concerning some thing t whether it is a member of the class determined by P. There are three distinguishable cases depending upon whether the definiens chosen is a sufficient, necessary or associative definiens for P. If it is a sufficient definiens for P then it is definitely the case[1] that a member of the class determined by the definiens is a member of the class determined by P. Thus, if the definiens is sufficient for P, let us say that if t is a member of the class determined by the definiens that it is *absolutely* a member of the class determined by P. If any t is a member of the class determined by either an associative or a necessary definiens for P, then it does not definitely follow that it is a member of the class determined by P. Thus, if its definiens is associative or necessary for P, let us say that if t is a member of the class determined by the definiens that it is *conditionally* a member of the class determined by P.

Suppose we ask concerning some thing t whether it is a non-member of the class determined by P. There are again three distinguishable cases depending upon whether the definiens chosen is a sufficient, necessary or associative definiens for P. If it is a necessary definiens for P then it is definitely the case that a non-member of the class determined by the definiens is a non-member of the class determined by P. Thus, if the definiens is necessary for P let us say that if t is a non-member of the class determined by the definiens that it is *absolutely* a non-member of the class determined by P. If any t is a non-member of the class determined by either a sufficient or an associative definiens for P, then it does not definitely follow that t is a non-member of the class determined by P. Thus, if the definiens is associative or sufficient for P, let us say that if t is a non-member of the class determined by the definiens that it is *conditionally* a non-member of the class determined by P.

It should be noticed that these notions of absoluteness and conditionality apply not to constructed languages analogous to ordinary language and not to attempted analytic definitions but to the vague predicates of ordinary language itself.

[1] That is, " definitely the case " for a user of the language in question.

3. The notions of absoluteness and conditionality are useful in reasoning in which it is attempted to characterize certain things either as very good or as very bad. One illustration of their usefulness is as follows.

There has been in philosophy considerable discussion concerning what it means to say that a sentence makes a cognitively meaningful assertion.[1] This question has usually been answered by means of analytic definitions of the predicate 'cognitively meaningful assertion'. In other words it has been suggested that the vague predicate 'cognitively meaningful assertion' be made to specify a determinate class of things, namely assertions, by equating it with a more precise predicate, or "meaning criterion". And, as with most vague predicates, it is possible to choose from among several different definiens for 'cognitively meaningful assertion'. Let us consider three such definiens :

(a) true or false but not both.

(b) analytic or self-contradictory or capable, at least in principle, of experiential test.

(c) translatable into an empirical language.[2]

Now 'cognitively meaningful assertion' definitely implies 'true or false but not both', definitely is implied by 'translatable into an empirical language' and neither definitely implies nor is definitely implied by 'analytic or self-contradictory or capable, at least in principle, of experiential test'. Therefore (a) is a necessary definiens, (b) an associative definiens, and (c) a sufficient definiens for 'cognitively meaningful assertion'. Therefore, if a sentence is not : true or false but not both, it is absolutely not cognitively meaningful ; however, if it is a non-element of (b) or (c) it is only conditionally not cognitively meaningful. Likewise if a sentence is translatable into an empirical language it is absolutely cognitively meaningful ; however, if it is only a member of (b) or (a) it is only conditionally cognitively meaningful.

Now suppose that a contemporary empiricist finds that a certain assertion A of ordinary language does not satisfy the meaning criterion (c). It does not follow from the fact that A is conditionally meaningless by (c) that it has therefore only emotive import, poetic meaning, or pictorial significance.[3]

[1] For a survey of and bibliography to much of this discussion see *Revue Internationale de Philosophie* 11, January, 1950, and especially "Problems and Changes in the Empiricist Criterion of Meaning" by Carl Hempel in the same issue.

[2] For this latter criterion see the article by Hempel mentioned in the preceding note.

[3] For example, some of the writings of Carnap, Schlick and Ayer suggest that language which does not satisfy the empiricist meaning criterion has only an "expressive" or "poetic" function. See, *e.g.*, R. Carnap, *Philosophy and Logical Syntax*, pp. 26–31.

That is, an assertion can be conditionally cognitively meaningless but still be cognitively meaningful. However, if A is judged cognitively meaningless by (a) it is absolutely cognitively meaningless, it cannot make a cognitively meaningful assertion, and it *may* be correct to conclude some such thing as " it has only an expressive function ".

On the other hand if an assertion A is a cognitively meaningful assertion by (a), it cannot be concluded from this fact of its conditional cognitive meaningfulness either that it states or denies a matter of fact, or that it can be used in scientific undertakings, or that it is cognitively meaningful without qualification. It may be possible to draw these conclusions if A is also cognitively meaningful by (c) *i.e.*, absolutely cognitively meaningful.

Thus the notions of absoluteness and conditionality can free both empiricists and anti-empiricists from some of their characteristic reasoning errors.

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RAMSEY ON RULES

By Thomas McPherson

1. THIS paper is about Ramsey's view of causal laws as *rules*. Ramsey says :
    " Variable hypotheticals or causal laws form the system with which the speaker meets the future. . . ."

" Variable hypotheticals are not judgments but rules for judging ' If I meet a ϕ I shall regard it as a ψ '. This cannot be *negated*, but it can be *disagreed* with by one who does not adopt it."[1]

And ten pages later he says :

" . . . when we assert a causal law we are asserting not a fact, nor an infinite conjunction, nor a connection of universals, but a variable hypothetical which is not strictly a proposition at all, but a formula from which we derive propositions ".

A question of interpretation arises at once. Did Ramsey mean what he said, or was he a careless writer of English ? " If I meet a ϕ I *shall* regard it as a ψ ", which is what he says, seems

[1] F. P. Ramsey, *Foundations of Mathematics*. p. 241.

to be a statement of fact. " If I meet a ϕ I *will* regard it as a ψ "
would be a statement of intention. And it makes some difference
to the understanding of Ramsey which way it is taken.

Are we to take him as having meant what, grammatically,
and Fowler's complications aside, he would be considered
as having said ? It is tempting to interpret him in the other way ;
but then one can never get over the thought that whatever he
may be *supposed* to have meant this view does not give us what
he seems clearly to have *said*. So in what follows I shall take
Ramsey's " shall " literally ; and assume that he did not mean
to say " will ", but did indeed mean what he turns out to have
said. " If I meet a ϕ I shall regard it as a ψ " is, then, a state-
ment of fact, not a statement of intention. It is a statement
of " psychological fact " about what my behaviour *is* or *will
be*, not a statement of how I *propose* or *intend* to behave.

There is perhaps another unclearness in the first quotation
from Ramsey. What is " If I meet a ϕ I shall regard it as a ψ " ?
Does Ramsey mean this as the form (or a form) that rules them-
selves take, or as the form (or a form) of the propositions that
can be derived from rules ? The second, one is sometimes
inclined to say. But at other times one wants to say that the
first is what he means. I think there is no harm in holding that
he may mean both. Take it that he means the first (whether or
not he means the second as well), then consider this :

"All ϕ-things are ψ-things " (causal law)=" If anything is
a ϕ-thing it is a ψ-thing " (variable hypothetical)=" If I meet
a ϕ-thing I shall regard it as a ψ-thing " (Ramsey's rule).

Ramsey, I think, might want to say that these *equivalences*
hold throughout. And I think he might be as willing to look on
the last as itself a *rule* as he would be to call it a proposition (it is
really a propositional function as it stands, of course) " derived
from " the second or first. The third is " derived from " the
second or the first, but it is itself a sort of rule from which we
can derive propositions, viz. propositions which have this
form but in which the blanks marked by " ϕ " and " ψ " are
replaced by appropriate terms. (And even such propositions
themselves can be understood as rules.)[1] It does not really
matter for our purposes that we should distinguish between
rules and propositions derived from rules; and I shall therefore
usually treat Ramsey's " If I meet a ϕ I shall regard it as a ψ "
as itself a rule, which it certainly is on one level anyway. And

[1] As they seem to be by Mr. Kneale, for example, who gives, " Whenever you find a
dodo assume that it has a white feather in its tail," as an example not of a proposition
derived from a rule but as itself a rule. He has changed Ramsey's " I " to " you," but this
difference (which is discussed below) is not of importance *here*.

the important question of what precisely " derived from " may mean here need not worry us either.

2. Now Ramsey, I think, is often interpreted as saying that he wants to translate causal laws into the form, " When you meet a ϕ expect it to be a ψ ", or something of that sort; anyway, into something expressed in the imperative mood. But he does not say this. And there is a difference between rules expressed in the indicative and rules expressed in the imperative. (Strictly speaking, rules are *never* expressed in the imperative. But let us assume for the moment that they can be.)

"All persons entering this room shall close the door after them ", painted up on the door panel, looks much less natural than " Close this door after you ", or " Please close the door after you ". But, on the other hand, " No bicycles may be left in this entrance ", or " Bicycles must not be left here," sounds, to me anyway, *more* natural than " Leave no bicycles here ". (Though, again, " Leave no litter " is perfectly all right.)

The differences in these examples can be accounted for by saying that we choose the forms we do for reasons of style or emphasis. Perhaps other sorts of reasons may apply to other sorts of contexts. But these examples will do to show that, for whatever reason, some rules when put in the imperative somehow do not sound right. Possibly causal laws interpreted as rules is one of these. Ramsey, certainly, when he interpreted causal laws as rules, did not interpret them as rules in the imperative.

Mr. Kneale seems to be one who considers that Ramsey *did* interpret causal laws as rules in the imperative (or, perhaps, as rules from which imperatives can be derived). In his *Probability and Induction* he discusses Ramsey together with Schlick. Although he does not say so, I think his discussion implies that the example he gives of such a rule or formula (Mr. Kneale says " prescription ") would be accepted by Ramsey and Schlick equally. The example is : " Whenever you find a dodo assume that it has a white feather in its tail ". Whatever Schlick may have said, I do not think Ramsey would have put a " prescription " in this form. Ramsey might have said (if he believed the general statement that all dodos have white feathers in their tails), " If I find a dodo I shall assume it to have a white feather in its tail ". But I do not think he would have said, " Whenever you find a dodo assume that it has a white feather in its tail ".

Now the difference between these ways of expressing a rule is obvious. Mr. Kneale makes it very obvious by saying " you " instead of " I ", as well as by putting it in the imperative

mood. It is natural that he should say " you " and not " I " ; for we do not often *give orders to ourselves* in this explicit way.[1] But why should Mr. Kneale think in the first place that these rules *do* give orders (or express commands) ? Ramsey's rule tells *me* what *I shall* do if I meet a ϕ—viz. regard it as a ψ. Mr. Kneale's tells *you* what *you ought* to do if you meet a ϕ— viz. assume it to be a ψ.

This difference in grammatical mood and person is not unimportant It makes Mr. Kneale's criticism of prescription theories inapplicable to Ramsey. Mr. Kneale says :—

" The practice of scientists in the formulation of natural laws is not adequately described as the making of rules for the guidance of their expectations. When they have formulated a law of nature, scientists consider themselves entitled, not only to make predictions about the unobserved, but also . . . to assert unfulfilled hypothetical propositions implied by the law. If the sentence which purports to formulate a law gives only a general rule, what is derived from it can be no more than a command or injunction. It is absurd, however, to treat an unfulfilled hypothetical statement as an injunction to expect something."[2]

But it is just *not true* that, as Mr. Kneale says, " if the sentence which purports to formulate a law gives only a general rule, what is derived from it can be no more than a command or injunction ". *Ramsey's* rules are not used by him for deriving commands or injunctions. Ramsey (in spite of his phrase, " the system with which the speaker meets the *future* ") would surely not have objected to the putting of his rule sometimes in the form of an unfulfilled hypothetical : " If I had met a ϕ I should have regarded it as a ψ."

This is the place to return to the point that I made above, but then shelved—the point that rules are in any case *not* expressed in the imperative, and so it is just wrong to say, as is sometimes said, that a " rule " theory of causal laws involves translating them into commands or injunctions. If it does involve this it is not a rule theory. To revert to my own earlier examples. "All persons entering this room shall close the door after them ", is a rule. The alternative, " Close this door after you " (or " Please close the door after you "), is not a rule but

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[1] Though if we interpret Ramsey's rule as a statement of *intention* there is perhaps not this obvious difference. It has been pointed out to me that statements of intention seem to play, in the first person, a role in some ways analogous to that played in the second person by commands. (But we need to remember that the role they play is not in *all* ways analogous).

[2] W. Kneale, *op. cit.*, p. 77.

a request. Mr. Kneale's example, "Whenever you meet a dodo assume that it has a white feather in its tail ", is not a rule, but again a request, or possibly an order, or possibly a piece of advice.

3. We use the word " rule " in different ways. Two uses that are of interest here are these : (*a*) in the phrase, " to make a rule " (in drawing up the rules for a new game, for example) ; and (*b*) in the phrase, " to make it a rule " (which is like the use of " practice " in such sentences as, " I always make a practice of doing so-and-so "). Rules of chess are of the first kind. The second kind is exemplified in, say, the remark, " I always make it a rule to say a civil word to him whenever we meet ". Now we would not say, " I always make it a rule to move my bishops diagonally when I play chess. What do you do with *your* bishops ? "

I suggest that Ramsey's use of " rule " is nearer (*b*) than (*a*). I do not at all mean that it is the same as (*b*), but only that it is somehow more like (*b*) than it is like (*a*). As it is neither (*a*) nor (*b*), it is, presumably, a third use (*c*). But what, in detail, this third use may be does not concern us. All I am interested in is its comparative likeness to use (*b*) and comparative unlikeness to use (*a*). Ramsey's, " If I meet a ϕ I shall regard it as a ψ ", is more like, " I always make it a rule to say a civil word to him whenever we meet ", than it is like, "A bishop shall move diagonally any number of places." But he has often been interpreted as saying something like (*a*).

There is a marked " psychological " or " attitudinal " flavour about Ramsey's view of causal laws. Consider, for example, the first sentence in the quotation from him at the beginning of this paper. And consider, too, his view that causal generalisations are those that we *believe*, and that we trust to guide us in new situations. And there is his remark about " the *habit* of singular belief " involved in believing that all men are mortal. All this obviously links with the second use of " rule " mentioned above.

Ramsey denies that his view is Humean (in a note at the end of the essay on " General Propositions and Causality "), but there is a Humean ring about much that he says. This is, of course, not to deny the *difference* between Hume and Ramsey ; the difference that makes Ramsey's theory the important development that it is. Hume assumed that causal laws are fact-stating. Ramsey says they are not fact-stating ; that they are not propositions at all, but rules or formulae, " from which we derive propositions ". This is the logical side of Ramsey.

But there is a psychological side as well. And Ramsey does talk at times as if he wanted to account for " the idea of necessary connexion " in a Humean, psychological way.

But we have been understanding Ramsey's, " If I meet a ϕ I shall regard it as a ψ," as a statement of fact (of " psychological fact ") ; and can it not now be seen that this interpretation conflicts with his clearly expressed view that causal laws are not fact-stating ; and is this not an argument in favour of interpreting his rule as a statement of intention and not as a statement of fact ? Perhaps. Or, alternatively, have we not now an argument in favour of understanding Ramsey's, " If I meet a ϕ I shall regard it as a ψ," not as itself a rule, but as a proposition derived from a rule ? Perhaps.

4. Another point. Ramsey says, in one of the passages that I have quoted, that a rule cannot be negated, but can be disagreed with by one who does not adopt it. This seems to me wrong. Ramsey's rules *can* be negated. Their negation *seems* to be trivial, but it is possible. We negate Ramsey's, " If I meet a ϕ I shall regard it as a ψ," by saying, " But I won't ". (Disagreement would be, " No you won't.") To put it more strictly, the negation would be, " If I met a ϕ I shan't regard it as a ψ ". It may be said that this sort of negation is not what Ramsey means, and that to point it out is simply to evade the issue he is raising : Ramsey's saying that rules cannot be negated is only another way of impressing his view that causal laws are not fact-stating. But nevertheless they *can* be negated, in the way I have shown, and it is important to see that this is so. It is important for the following reason.

Ramsey, in saying that rules cannot be negated, lays himself open to criticism. I suppose it would be agreed that some suggested causal laws have been, and others may be, *wrong* (or mistaken). This seems to involve that they can be negated (that is, that one can significantly state their contradictories). Now, if Ramsey's view of causal laws as rules is to be satisfactory, it would seem that some rules (those that " wrong " causal laws can be translated into, or are the statements of) are themselves somehow wrong, and so must be capable of being negated if we are to be able to *say* that they are wrong (one is inclined to use " false " instead of " wrong ", but this seems not quite the best word). It looks, then, as though Ramsey's treatment of causal laws as non-negatable rules breaks down in cases where the causal laws in question are " wrong " ; and *any* causal law *may* be wrong. However, if I am right in saying that Ramsey's rules *can* be significantly negated, then this criticism

will not apply. The negation of rules turns out not to be so trivial as it looks. Ramsey ought not to have said that rules cannot be negated ; because they *can*, and because, if he had not said it, his version of the " rule " theory would be more plausible than it is.

5. What I have tried to do in this article is to interpret Ramsey, not criticise him. If my interpretation of him is right, I have saved him both from Mr. Kneale and from himself. But perhaps I ought to end by pointing out—what is obvious enough—the chief thing that is wrong with his view as I have interpreted it.

Ramsey seems to have got to his theory by combining Bradley's and Russell's interpretation of general propositions as variable hypotheticals with a Humean view of the idea of necessary connexion. Though, in fact, it was not Hume himself that Ramsey both absorbed and reacted against, but Hume as amended by Mr. Braithwaite.[1]

Hume said that we get the idea of necessary connexion from observing sequences of A and B. After a while we come strongly to *expect* B to follow A. The idea of there being some sort of *necessity* in B's following A is explained by this strong expectation in the observer ; indeed, it *is* just this strong expectation in the observer. Mr. Braithwaite thought this was not good enough. In his paper on " The Idea of Necessary Connexion " he says that he thinks there is an *awareness* in the observer that he has the habit of expecting B to follow A ; and also that the expectation is something that is shared by other people. Hume's explanation is not sufficient because he leaves these things out. " It is my awareness of these beliefs in myself, and my knowledge that other people share them that is the foundation for my ' idea of necessary connexion ' ".[2]

Ramsey reacted against the Humean view, which came to him through Braithwaite, to the extent of denying that general propositions are fact-stating. With Bradley he interprets them as variable hypotheticals, and then looks on the variable hypotheticals as non-fact-stating *rules*. But he keeps the Humean explanation of the idea of necessary connexion, and understands it in a highly individualistic way as lying in *my* expectation (or assumption) that when *I* meet an A it will turn out to be a B.

But this will not do at all. One does not need to return to a " fact-stating " view of general propositions in order to see

[1] R. B. Braithwaite, " The Idea of Necessary Connexion ", *Mind*, 1927 and 1928.
[2] *Op. cit.*, p. 476.

that "All A's are B" says more than "If *I* meet an A *I* shall assume it to be a B". And other and later versions of the rule theory avoid this shortcoming of Ramsey's.

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ACHILLES AND THE TORTOISE

By L. E. THOMAS

MR. BLACK'S and Mr. Taylor's articles furnish one more demonstration that a resolution of Zeno's paradox is not to be got by attacking the problem as though it were one of explaining how a body may move from one point to another in an infinite series in a finite time.

What is the paradox? The paradox consists in the fact that we know that Achilles does overtake the tortoise and we know also that space and time are both infinitely divisible. These two pieces of knowledge are held to be incompatible with each other and their conjunction to generate a paradox.

I think we must admit that Achilles does in fact overtake the tortoise without the least difficulty and also that space and time are infinitely divisible. But this does not commit us to a paradox. To suppose that it does derives from a fallacy that infects many discussions of time.

Suppose that Achilles (A) and the Tortoise (T) start to race at the same moment of time, A starting from point x and T from point y which is in advance of x by a finite distance along the direction of the race. Then in a finite time, provided that A can run faster than T, A will overtake T. But this is impossible, it is held, because by the time that A has reached point y T will have reached point z which is less in advance of y than y is in advance of x. By the time A has reached point z T will have reached a point further on which is less in advance of z than z is in advance of y. And this process goes on *ad infinitum*, the interval between the two contestants getting progressively smaller but never becoming zero. Hence Achilles must always remain a finite distance behind the tortoise.

But if we accept the presuppositions underlying this argument the argument itself proves too much. Not only does A never overtake T; A does not even approach T. In fact neither A nor T can move at all. For, to consider the case of A, before

A can reach point y there is an infinity of points to be traversed between x and y; and between x and any one of these there is a further infinite series of points, and so on *ad infinitum*. In other words there is no next point to x to which A can move.

How then is A to overtake T? To ask this question is to commit the fallacy to which I have referred. For if we mean by 'A' Achilles at point x at the instant t, when the race started, then A in that sense (call it, A) never can overtake T.

Suppose a completely timeless, but spatial, Achilles all set at point x to start the race. Then such an Achilles does not exist wholly at point x. If one of his feet is at point x, then it is so only by courtesy; for it is only a cross-section of that foot which coincides with x. The *whole* foot is never at a point but covers an infinite series of points. So also a merely spatial Achilles never exists at any one spatial point but at a place which includes an infinitude of spatial points.

Consider now the temporal and spatial Achilles. The Achilles at point x at the instant t_1, when the race started is a fiction. The temporal Achilles can no more exist at an instant than the merely spatial Achilles can exist at a point. The Achilles who starts to race from point x at the instant t_1, is the Achilles who just before that set himself ready for the race, and who before that accepted the challenge to race the tortoise, and so on and so on. This is the Achilles too who at instants t_2, t_3, ... t_n gradually approaches point y and eventually overtakes the tortoise. It is the Achilles of whom we can write a biography. A_1, that is Achilles at point x at the instant t, has no biography. It is not surprising therefore that his having overtaken the tortoise at a specifiable point at a specifiable instant cannot be an item in his biography. The whole Achilles, the subject of a biography, is the integration of the series A_1, A_2, A_3, ... A_n, each member of which is an abstract cross-section of the essentially durational being which is Achilles.

To make the position clearer, consider Achilles for a short portion of his course. As A_1 he is at point x at the instant t_1. At the same time there is a series of points $a, b, c \ldots$ in advance of x. There will also be a series of stages in the career of Achilles at times $t_2, t_3, t_4 \ldots$ represented by A_2, A_3, A_4 ... But A_2, A_3, A_4 ... will not coincide in turn respectively with $a, b, c \ldots$ for $a, b, c \ldots$ are fictions as much as are A_2, A_3, A_4, etc. Since the racecourse is itself an enduring entity, then any arbitrarily chosen points in it will also have durations. Hence if a, b, c, etc. are points in the racecourse at t_1, and if A_2 A_3, A_4, etc., are successive stages in A's career at t_2, t_3, t_4, etc.,.

then A, will coincide with point a_1, A, with b_1, A, with c_1, and so on. In other words, no point of space arbitrarily chosen, *at one time* can occur at any other time. I believe a similar argument can be given to shew that no more than one arbitrarily chosen point can be allocated to any one instant, in which case the argument in this paragraph is somewhat misleading though not seriously so for the present purpose, but I do not wish to pursue the matter further here.

The fallacy I have referred to as generating the paradox we have been considering is briefly this. It is to suppose that any entity that exists for any length of time is *essentially complete* at any given moment in that period. It is to suppose that such an entity exists at any moment like a Leibnizian monad big with its own future, so that its history at any future moments consists merely in unrolling what was already there but rolled up. Thus the universe might be imagined as a machine wound up at the beginning, its subsequent history consisting in its running down. The kind of picture one seems to have in mind when one finds the Achilles and Tortoise contest paradoxical is something like this. The progress of the essentially complete Achilles is imagined as a series of flea-like hops from one point to another. A less misleading procedure would be to imagine the progress of Achilles as a line covering the whole series of points actually traversed in the race ; only the line must obviously not be imagined as a *growing* line, for we should then have the same difficulty of explaining how the growing end of the line advances from one point to the next. This fallacy which lies at the root of the paradox has been aptly called by Paul Weiss the *fallacy of essential completeness*.

Similar considerations to the above are relevant to the solutions of the Stadium argument and the Flying Arrow paradox.

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THE MEANING OF ' SURVIVAL '

By D. A. REES

I SHALL try to investigate briefly a problem affecting the nature of their enquiries which has, I fear, frequently received insufficient attention from psychical investigators. I shall say nothing of any religious or metaphysical conception of immortality, confining myself to that notion of survival which psychical

investigation seems, when it embarks on the question, to pre-suppose ; and I shall assume provisionally that Ryle's analysis of mental states in terms of actual or hypothetical bodily activi-ties is not fully adequate so far as at least some mental states are concerned, in particular the having of mental images. This is, I think, what we tend naturally to believe, though it may be that the points to which I shall draw attention point really in his favour. A further assumption, I shall make is that it makes sense to talk of a disembodied existence[1] and that, when a person dies, we can imagine the series of his mental states to continue, so that, while until he dies that series may be presumed to have physiological correlates, it may continue when such correlates no longer exist. The question I wish to raise is whether it makes sense to say that such a disembodied mental state exists at any particular time, and if so under what circumstances. Problems of verification are not the only ones which present themselves, and apart from them I have not been able to formulate any sense in which it is possible to speak of a mental state as existing at a particular time without some implicit reference to physical objects. But I am not suggesting that no such sense is possible.

The state of surviving consciousness normally conceived is one resembling a dream, but a dream from which there is no awaking. It is also a dream without the usual physiological correlates of sleeping and dreaming. By " X is sleeping ", " X is awake ", " X is conscious ", " X is unconscious ", we do not always mean the same thing. A doctor notices the quivering eyelids of the patient, and says that he is now conscious : it would scarcely make sense to say " He shows none of the signs of consciousness, but perhaps he is conscious ", or " He shows all the signs of consciousness, but perhaps he is not "[2] But when we speak of consciousness and mental states we are not always speaking in the same way as the doctor ; and it is clear that we are not when we speculate about the consciousness of a dead person.

The sequence of mental states in dreams cannot be marked off in any sequence of regular intervals—on any sort of time-scale—by any criterion internal to the series, as was noted by Bergson in L'énergie spirituelle. Their duration, as immediately observed, is baffling in the extreme. But, despite this, and despite the absence of any definite relation between their apparen and their temporal duration, it makes sense to say that A is

[1] Cf. C. Lewy, ' Is the Notion of Disembodied Existence Self-contradictory '? (*Proc. Arist. Soc.*, 43, 1942-3, pp. 59-78.
[2] Cf. Eliot Slater in *The Physical Basis of Mind*, ed. P. Laslett (Oxford, 1950).

dreaming now, both (i) because, if awakened, and perhaps subjected to suitable treatment, he would be able (if the proposition is true) to recall his dream, or would act in ways which we should describe as the effects of his dream, and (ii) perhaps in virtue of some physiological correlate. His series of dream-images can, in other words, be related to time in the physical world. But it is altogether different if the person in question is dead. What sense does it make to say that the mental states of B, who died yesterday, are continuing now ?

Cases may occur, or at least be imagined, in which the functions of life cease only to be resumed later, and therewith the normal characteristics of consciousness may recur, much as when we awake from sleep or from a faint ; and any intervening mental states can then be dated within fixed limits. But whatever problems are involved differ from those which arise in normal discussions of survival. What of the latter?

*   *   *

(*a*) There are the psychical phenomena which constitute alleged evidence for survival, but they are events in the physical world observable by living beings, or events in the consciousness of living beings. Are we to say, then, that the survival of a dead person consists simply in actual, or actual and possible, events in the physical world or in the consciousness of living beings? Such an analysis would be particularly attractive to one who adopted Professor Ryle's view of the mind. But this " survival " is different from any series of mental states experienced by the dead person, and if the latter is what we normally mean by " survival " the term is inappropriate here, and we should perhaps do better to use some such phrase as " *post mortem* phenomena ", inelegant though that is.

(*b*) We may interpret A's survival in terms of his consciousness of physical events, or of mental states of living beings, occurring now (whether his medium of awareness be telepathy or some other), or in terms of the possibility, if certain conditions were fulfilled, of his awareness of them. In that case the term " survival " will be rather more appropriate, whether the evidence for the belief be good or bad. Nevertheless, there are complications. In the world as we know it, my awareness of an event may be—normally will be—subsequent to that event ; and of two events, X and Y, X may occur before Y but I may be aware of Y before I am aware of X ; as when there are two radio loudspeakers in a large field and I am standing nearer to one than to the other, so that I hear sounds from it before I hear

sounds from the other which are "objectively" earlier. Let us apply this to the consciousness of a dead person, A, whose series of mental states is taken not to be terminated by death, and suppose that among his mental states which do not occur before death there are some which include awareness of physical events, or of states of consciousness of living beings, which A did not possess before death. But even within our living experience, as we have seen, to say that A is conscious now is not to ascribe to him consciousness of an event which is occurring now; so similarly to say that in A's disembodied consciousness there is an awareness of events, physical or mental, in our world will not be to say that he is aware of them at the same time, or even in the order in which they occur. Further, the date at which A becomes aware of an event may differ from that at which we become aware of his awareness; so that we have to distinguish t_1, when the event takes place, t_2, when A becomes aware of it, and t_3, when we become aware of A's awareness, and of these only t_1, and t_3 are directly investigable by us; so that any determination of t_2 on the basis of t_1 and t_3 will, if further supporting evidence is not available, be purely conventional. We can, if we like, and if the evidence seems to justify it, adopt such a convention; or we may prefer to speak simply of A's "*post mortem* states" without committing ourselves to the belief that a particular date could be assigned to any of them. Any justification for adopting the former alternative will depend in part on considerations to which the following paragraph will call attention.

(*c*) Some who talk about survival may mean (*a*), some (*b*), but probably many have meant something not covered by either. Are there any other senses of "survival" which can be made at all precise, and which can be employed with profit in psychical investigation? I doubt if any sense which goes materially beyond (*a*) and (*b*) can be of much importance for psychical research, but, even if we decide to mean something different, it will still be difficult to speak of a time which is not correlatable, either directly or indirectly, with physical occurrences. According to modern relativity physics, in any statement of time there is implicit a reference to some spatial coordinates, and so a time for which no such reference is possible will not be a time in the normal sense at all[1]. This, incidentally, is a problem of which Dr. Lewy's paper, mentioned above, does not take account. We may conceive a mental state to have a

[1] Cf., *e.g.*, G. J. Whitrow, *The Structure of the Universe* (1949), pp. 55-6, and ch. iv, or A. Milne in *Philosophy*, 25 (1950), pp. 68-72.

spatial correlate without the presence of a physical body which occupies that space, though no doubt the conception is strange, and it is difficult even to imagine methods by which instances of it could be satisfactorily verified. But (i) it is difficult to speak of such a state of mind as having a spatial correlate unless it is an awareness of physical events, or of mental events which have a physical correlate, or is in some way bound up with such awareness, as being an accompanying feeling or emotion (*e.g.*, can a pain which is not the pain of a physical body be spatially located if it is not connected with an awareness of something physical ?), and in this case the state of mind in question will come under heading (*b*) above ; and (ii) unless it has some spatial correlate it seems that we cannot speak of a mental state, or anything else, as existing at a certain time ; and it will scarcely make sense to speak of a spatial location without reference, direct or indirect, to physical bodies.

We can certainly speak without physical or spatial reference of one state of consciousness as following another, but can we then say by how much it follows it, or at what time either occurs? Talk about survival seems usually to presuppose the latter, though it may be that that is both unnecessary and mistaken. What we are left with are (i) *post mortem* phenomena, (ii) a series of states of consciousness imagined to be perhaps not terminated by death, (iii) an attempted correlation of some of (i) with undetermined (and perhaps indeterminable) corporeally unattached members of (ii). It seems further that a state of consciousness, like other things, can have an objective temporal correlate if only it has a spatial correlate (and *vice versa*) ; but how far, or in what ways, this is relevant to (ii) I do not know. One could certainly suppose states of consciousness to be capable of correlation in accordance with some other form of duration than ours, but of this we know nothing.

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ON SYNONYMY OF WORD-EVENTS

By Beverly Levin Robbins

DR. GOODMAN has suggested[1] that a difference of meaning between two predicates is always accompanied

by a difference in their primary or secondary extensions, where a secondary extension of a word is the extension of a compound having that word as a component. He argues that for every pair of predicates there is a corresponding pair of compounds, formed by identical additions (*e.g.* "—description ") to the two predicates, which do not apply to just the same things. Hence if identity of primary and secondary extension is adopted as a criterion of synonymy, then two different predicates, while possibly exhibiting some degree of likeness of meaning, never have the same meaning.

In a note on Dr. Goodman's paper,[1] Dr. Rudner claims,

" . . . if one takes the point of view of a rigorous nominalism, that not words or statements, but only inscriptions or parts of inscriptions have meaning, then even more obviously will no two inscriptions or parts of inscriptions have the same meaning. Indeed, if one takes simply the position that inscriptions and parts of incriptions are meaningful, one can maintain that no ' repetitive ' inscription is analytic ; for no two of its constituent parts have the same primary and secondary extensions " (p. 117).

I find this remark puzzling. What should be taken as appropriate compounds to show that two predicate-inscriptions (or better, predicate-events[2], say I_1 and I_2, differ in secondary extension ? If I_1 and I_2 themselves are to be constitutents of the compounds, then they must actually exist or have existed as so many marks or sounds within these compounds. Each can exist as part of at most one compound, since the same event cannot occur at different places or at different times. Thus I_1 or I_2 has either no secondary extension, if it does not happen to occur compounded, or exactly one secondary extension. In general, if we stipulate that the compounds corresponding to two predicate-events be formed by additions to the predicate-events themselves, then most predicate-events, being uncompounded, will lack secondary extension. Among such predicate-events, those with identical primary extensions will be synonymous, since they will also have the same secondary extensions by virtue of having none. Thus a literal application of Goodman's principle to inscriptions has consequences quite different from those Rudner expected : too many pairs, *e.g.* any uncompounded " centaur "-event and " unicorn "-event, will have the same meaning.

[1] Richard Rudner, "A Note on Likeness of Meaning ", *Analysis*, Vol. 10, pp. 115–118.
[2] " Event " seems preferable to " inscription ", since different temporal parts of the total history of a mark may be different words. For example, if a " No Parking Here " sign is moved about, certain different temporal parts of the " Here " are names of different places. Cf. Nelson Goodman, *The Structure of Appearance*, Harvard University Press, 1951, Ch. XI, §2, esp. p. 291.

Following Goodman, where two sign-events would ordinarily be regarded as instances of the same sign-design, I shall call them *replicas* of one another.[1] Thus, for example, any " centaur "-event is a replica of itself and of every other " centaur "-event. The relation of *being a replica of* is reflexive, symmetric, and transitive. Now Rudner may prefer to construe a statement like " I_1 occurs in the compound C_1 " as saying elliptically that some replica of I_1 is part of some replica of C_1. But then every two predicate-events that are replicas of each other occur in exactly the same compounds. Consequently, if two such predicate-events have the same primary extension, they will also have the same secondary extensions. The two " rose "-events in Rudner's example "A rose is a rose " will, contrary to his contention, have the same meaning.

Thus Rudner seems mistaken in supposing that if Goodman's proposal for testing synonymy is incorporated within a nominalistic theory of language, then every two word-events will differ in meaning.

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[1] *Ibid.*, Ch. XI, §2, esp. p. 290. For the terminology of " sign-event " and " sign-design ", cf. Rudolf Carnap, *Introduction to Semantics*, Harvard University Press, 1946, Part A, §3.

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